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ABSTRACT

Ethnography is coming to be much discussed in education. A definition of ethnography is, however, an elusive and complicated question. Anthropologists do not themselves have a unified conception of ethnography, especially of ethnography in relation to the study of institutions of our own society, such as education. One difficulty with the notion of ethnography is that it may seem to be a residual category, associated with the study of people not ourselves and with the use of methods other than those of experimental design and quantitative measurement. Educational research has been traditionally dominated by these quantitative and experimental conceptions of research. Ethnography, which involves participation and observation, and which is systematic, comprehensive and topic oriented, could provide the opportunity for mutual relation of interaction between ethnographers and sponsors of educational research. With systematic and comprehensive information about the community to be studied in an educational context, ethnographers will be able to test hypotheses in the field of educational research. (Author/GC)

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What is Ethnography?

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What is Ethnography?[1]

I

"Ethnography" is coming to be much discussed in education. Often enough one hears some form of the question, "What is ethnography?" The National Institute of Education has commissioned a report to answer the question. All this might be puzzling to an anthropologist, especially to one with an interest in the history of the subject. If one traces the history of ethnography where it leads, one goes back centuries, indeed, to the ancient Mediterranean world, and the temporary rise and fall of ethnographic inquiry there, Herodotus being its most famous, but not only, exemplar. With regard just to the Americas, one can trace a fairly continuous history of the ethnographic reports, interacting with the posing of ethnological questions, from the first discovery of the New World. There is a considerable modern literature on the practice of field work, both in general and with regard to specific techniques, and more recently, ethics. A recent book addressed to ethnography in our own society (Spradley and McCurdy 1972) is being used, I understand, by teachers of composition to stimulate topics for their students. If ethnography is new to some in education, certainly it is not new to the world. When asked "What is ethnography?", would it not be enough to provide a short reading list, or to point to the discussion in some text of what research proposals often refer to as "standard ethnographic method"?

I fear not. Anthropologists do not themselves have a unified conception of ethnography. In particular they do not have a unified conception of ethnography in relation to the study of institutions of our own society, such as education. And

anthropologists are far from accepting or perfecting an integration of the mode of research they would consider ethnography with other modes of research into a society such as our own. And the changing intellectual context of the human sciences as a whole introduces new questions and sources of diversity.

Educational research has been dominated by quantitative and experimental conceptions of research. It is easy for anthropologists of a variety of persuasions to criticize such methods. It is often harder for them to state concisely the alternatives. "Ethnography" cannot be assumed to be something already complete, ready to be inserted as a packaged unit in the practices and purposes of institutions whose conceptions of knowledge and research have long been different. If there is not careful thinking through of underlying conceptions, and explicit attention to differences in them, "ethnography" may be a brief-lived fad in educational research. Or worse, partial or superficial conceptions may be taken up.

The true opportunity of the current interest in ethnography is to enter into a mutual relation of interaction and adaptation between ethnographers and sponsors of educational research, a relation that will change both. Because of my conception of ethnography, I see in this prospect a gain for a democratic way of life. The following sketch is offered because I do not know of a similar attempt to consider the issues raised here in brief compass. Many others must contribute from their own experience and outlook.

II

One difficulty with the notion of "ethnography" is that it may seem a residual category. It is associated with the study of people not ourselves, and with

the use of methods other than those of experimental design and quantitative measurement.

Clearly not everything that is not those two things should be considered "ethnography," but a positive definition is not easy to provide. A major reason for the difficulty is that good ethnography has been produced under a great variety of persons, some of it before there was a profession to train such people; and professional training has been very much a matter of the transmission of a craft and of learning by doing, by personal experience.

It has not helped that some people talk as if the key to ethnography were a personal psychological experience, rather than the discovery of knowledge.

It is clear that ethnography involves participation and observation. What should count as ethnography, what kinds of ethnography there are, may be more easily seen if we consider what makes participation and observation systematic--what, in short, counts as systematic ethnography.

The earliest work that we recognize as important ethnography has generally the quality of being systematic in the sense of being comprehensive. To be sure, any and all early accounts of travellers, missionaries, government officials, and the like, that may contribute information and insight about the culture of the peoples of the world, has been welcomed and gleaned for what it could provide. But when Ibn Khaldun or Father Sahagun are singled out, it is because they are both early and comprehensive. Their curiosity was not limited to curiosities; they had an interest in documenting and interpreting a wide range of a way of life.

Much of the early attempts to make ethnographic inquiry an explicit procedure reflect the desire to be comprehensive. These attempts are guides to inquiry, lists

of questions, of observations to make. They bespeak a stage of history when much of the non-Western world was little known to Europe, and when a variety of reasons, scientific, religious, practical, motivated some to seek more adequate knowledge. These guides to inquiry have in common a concern with all of a way of life (although their coverage may be unequal). What are the people of such and such a place like?

It was not long before there were explicit procedures that can be distinguished as topic-oriented. (Indeed, the Domesday Book and the inquiries of Sir William Petty (1623-97) share in this lineage.)[2] The great American example is Lewis Henry Morgan's questionnaires for recording kinship terminologies in the middle of the nineteenth century. It is worth pausing to consider the several aspects of Morgan's great work. First, he had a contrastive, or comparative insight: from his experience with the Iroquois Indians, together with his knowledge of classical Greece, he realized that there was a principle of kinship organization sharply contrasting with that familiar to contemporary Americans and Europeans. He sought then to determine the main types of kinship system and their locations throughout the world. Second, he needed systematic information, information not available except as he sought it himself. Hence his own travels in the Western United States and his relentless correspondence with those who could help. Third, he made use of his findings to formulate first a historical (Systems of Consanguinity and Affinity, 1870) and then evolutionary interpretation (Ancient Society, 1877) of the most general sort, of human development as a whole.

These three aspects of inquiry seem the essential ingredients of anthropological research proper, as distinct from inquiry that contributes to anthropology. Each

aspect may exist independently--a contrastive insight, a seeking of specific information, a general interpretation. Anthropology proper exists insofar as the three are united in a common enterprise. Ethnography is more than a residual technique, but the name of an essential method, when all three are united.

With time there has come to be a certain body of ethnographic inquiry that can be said to be hypothesis-oriented. To be sure, Morgan had a general hypothesis. But it seems reasonable to distinguish the kind of ethnography organized and guided by John and Bea Whiting, for example, to inquiry into socialization in several contrasting societies. The Whitings had attempted to come to general conclusions, testing hypotheses on a theoretical base, from the ethnographic literature that existed at the time. They found, as so many find, that their questions were more specific than the literature could answer. The sources were not detailed enough for their purpose, and not comparable enough. Therefore they organized a project to provide the detailed, comparable information they needed. Ethnographic teams (generally, couples) were trained in terms of a guide to the field study of socialization; sent into the field in several different societies for an extended period of time; kept in touch with through correspondence throughout the field study; and brought back to write up their results. (Six Cultures, by Bea Whiting, is a principal outcome). Like Morgan, the Whitings had insight into contrasting types of society; a need for specific information; and a general theoretical frame (in this case, psychodynamic) to which contrast and specifics were relevant.

All three types of ethnographic inquiry continue to coexist in anthropology today. There are still occasional discoveries of unknown peoples (as in the Philippines a few years ago), for whom comprehensive information has to be provided from

scratch. There are still many peoples, knowledge about whom has never been adequately systematized, for knowledge about whom has serious gaps. Fresh ethnography may be undertaken for first-hand knowledge as a basis from which to integrate all that is known, or to fill a gap in what is already known.

There are still discoveries of aspects of culture, or of perspectives on culture, such that the existing literature fails to provide much information. The "ethnography of speaking" is a case in point. The Human Relations Area Files, although rich in ethnographic data, simply did not contain much information on cultural patterning of speech, let alone information at all on the fundamental theoretical question, the functions of speech in the society. (Anthropological theory had taken for granted that the functions of speech were everywhere the same). When such a discovery or perspective comes to the fore, topic-oriented ethnography may be undertaken. One needs to find out something of the range of cultural patterning, once cultures are investigated from the new point of view.

Both comprehensive ethnography, and topic-oriented ethnography, lead to hypothesis-oriented ethnography. Given a substantial general knowledge of a culture, precise investigations can be planned. Indeed, hypothesis-oriented research depends on the existence of comprehensive ethnography, and can be fruitfully pursued only where the latter exists. Again, once something of the range of patterns for an aspect of culture is known, one begins to formulate more precise questions. Research may show the recurrence of a contrast in styles of speaking that can be called "direct" vs. "indirect"--but are the attributes of the two styles the same in each case? Are the functions the same? In the ethnography of speaking right now, one is

aware of broad contrasts, usually presented as dichotomies (cf. Bernstein's "elaborated" and "restricted" codes). This fact is a sure sign, I think, of the topic-oriented stage and of the need to proceed to the hypothesis-oriented stage.

III

What might the subject of schooling in America be like in this context? Clearly there is a great deal of information already in hand. It is not so clear that the information is obtained and analyzed in ways that permit all the insight possible into schooling. If schools were considered from the same standpoint as kinship systems or languages, the first question might be what kinds of schools are there? It would not seem informative enough to know that test scores were up or down in general across all American schools, if in fact the country contains schools of many different types. The point would apply even within a single city or district. Are the schools of District 1 in Philadelphia all alike? If they are different, how many different kinds are there? Probably at any level of consideration, one would not want to say that all were alike, nor that all were incomparably unique. In sum, one would recognize a question of typology, as central to analysis.

A useful typology has to be designed in terms of a particular purpose. Kinship is important to social life, and central to many societies, but even so, a classification and analysis of societies according to kinship is not the same as a classification and analysis according to religion. There are strong connections, but not invariant bonds, among the various sectors of a way of life, and so also, among the various sectors of schools. A typology of schools in District 1 in terms of verbal skills, questions having to do with literacy, would not necessarily be

the right typology for some other purpose. Conversely, and here is an essential point, a typology for some other purpose is not necessarily right for a concern with verbal skills.

This essential point is an example of a general consideration that divides many ethnographers from an experimental model, at least as that model is understood by them. For many ethnographers, it is of the essence of the method that it is a dialectical, or feed-back (or interactive-adaptive) method. It is of the essence of the method that initial questions may change during the course of inquiry. One may begin with the assumption that every community must have a pattern for the residence of newly-married couples that can be of only one of four types, yet discover that the community one is studying actually determines the residence of newly-married couples on the basis of principles one had not foreseen. (The illustration is an actual one. See W. Goodenough, "Residence rules," 1956).

The history of anthropology is replete with experiences of this sort. The general mission of anthropology in part can be said to be to help overcome the limitations of the categories and understandings of human life that are part of a single civilization's partial view. For many ethnographers, an essential characteristic of ethnography is that it is open-ended, subject to self-correction during the process of inquiry itself.

All this is not to say that ethnography is open-minded to the extent of being empty-minded, that ignorance and naivete are wanted. The more the ethnographer knows on entering the field, the better the result is likely to be. Training for ethnography is only partly a matter of training for getting information and getting

along. It is also a matter of providing a systematic knowledge of what is known so far about the subject. The more adequate this knowledge, the more likely the ethnographer will be able to detect the points in a situation that are important because they are truly novel, or because they bear on a debated issue; the more likely the ethnographer will be able to avoid blind alleys and pursue fruitful directions, having a ground sense of what kinds of things are likely to go together, what kinds of phenomena need minimal verification, what most.

One conception of this process is that of Kenneth Pike. Pike generalized the experience of linguistic inquiry. In order to discover the system of sounds of a language one had to be trained to record the phenomena in question and one had to know what types of sound were in general found in languages. Accurate observation and recording of the sounds, however, would not disclose the system. One had to test the relations among sounds for their functional relevance within the system in question. The result of this analysis of the system might in turn modify the general framework for such inquiry, disclosing a new type of sound or relation. Pike generalized the endings of the linguistic terms "phonetic" and "phonemic" to obtain names for these three moments of inquiry. The general framework with which one begins analysis of a given case he called "$etic_1$." The analysis of the actual system he called "emic." The reconsideration of the general framework in the light of the analysis he called "$etic_2$."

When ethnographic and linguistic inquiry are described in such terms, it may be easy to see the connection with general scientific method, and the exemplification of such method in the experimental sciences. For many ethnographers and lin-

guists the spirit of inquiry is indeed the same. The scale and conditions of inquiry in ethnography, nevertheless, impose essential differences in tactics. Perhaps the key to these differences is meaning.

For ethnographic inquiry, validity is commonly dependent upon accurate knowledge of the meanings of behaviors and institutions to those who participate in them. To say this is not to reduce the subject matter of ethnography to meaning, let alone to native views of meaning. It is simply to say that accurate knowledge of meaning is a sine qua non. The problem is obvious enough in the case of a language and culture we do not know. It is less obvious in the case of communities around us. Yet even though one may live nearby, speak the same language, and be of the same ethnic background, a difference in experience may lead to misunderstanding the meanings, the terms, and the world of another community. In Philadelphia, for example, a questionnaire was prepared by a person generally qualified by training and background. The purpose of the questionnaire was to find out what parents thought of a community-relations policy and person. The questionnaire was duly administered. The student administering the questionnaire discovered, by informal conversations with parents, that they interpreted the questions differently than the designer and the school. They distinguished between a playground (having equipment designed for children to use) and a playyard, but the questionnaire did not. When asked if they had had a chance to meet their School-Community Coordinator, they answered "no," because to them to "meet" would require having talked, and knowing by name, even first-name, not just having been introduced; but in terms of the questionnaire their "no's" were interpreted as "not having met." The student administering the questionnaire was distressed, but there was no way in the procedure of inquiry

for him to take account of what he had learned or to have what he had learned affect the presumed results (Abbott 1968).

Experiences of this kind make ethnographers distrust questionnaires, and quantitative results derived from them, if the meanings of the questions to those asked to answer are taken for granted in advance. Many ethnographers do use questionnaires, but questionnaires devised after sufficient participation and observation to ensure their validity.

The validity of knowledge about persons, families, neighborhoods, schools, communities in our country depends upon accurate and adequate knowledge of the meanings they find and impute to terms, events, persons, institutions. To an important extent, such meanings cannot be taken for granted as uniform, even within a single city or school district, nor as known in advance. The overt forms may be familiar--the words, the attire, the buildings--but the interpretation given to them is subject to shift, to deepening, to fresh connecting up. (It has been found that within a single small factory in Pennsylvania, those who worked in different parts had different terms for the same things (see Tway (1975)).

It is in the nature of meanings to be subject to change, reinterpretation, re-creation. One has to think of people, not as the intersection of vectors of age, sex, race, class, income, and occupation alone, but also as beings making sense out of disparate experiences, using reason to maintain a sphere of integrity in an immediate world.

All this is not to say that ethnography indulges in an infinite regress of personal subjectivity and idiosyncratic worlds. It has to be open to that dimension

of social life, because that dimension affects the reality of social life, and the success or failure of social programs. The point is to stress the necessity of knowledge that comes from participation and observation, if what one thinks one knows is to be valid. And all this is not to say that members of a community themselves have an adequate model of it, much less an articulated adequate model. All of us are only partly able to articulate analyses of our lives and their contexts. The meanings which the ethnographer seeks to discover may be implicit, not explicit. They may not lie in individual terms (words, objects, persons) that can be talked about, but in connections that can only gradually be discerned. The deepest meanings and patterns may not be talked about at all, because so fully taken for granted.

Here again the need to discover and validate in the given case is paramount. Our familiar categories of institutions, modes of communication, or the like, are an indispensable starting point (Pike's "$etic_1$"), but are never to be equated with an analysis of the organization of a local way of life. We necessarily distinguish speech and song, and as polar opposites, there may be speech with no musical quality, and singing without words. In our own musical traditions and in the cultures of the world, the interconnections and conceptions of these relationships of speaking and singing (and music generally) are various and diverse. Modern serious music includes such categories as Sprechstimme. The Maori of New Zealand consider the playing of the flute a form of speaking. It is especially these local nodes of connection, these community-specific ways of putting the encyclopedias of culture together, that cannot be assumed in advance of inquiry, and that can only be discovered through participation and observation over time.

We know that Philadelphia has newspapers, radio stations, television stations, libraries, books, comic books, magazines, inscriptions and plaques, narrators and joke-tellers. Without ethnography we can collect statistics as to production and distribution. Only with ethnography can we discover the connections among these things in the lives of particular kinds of people. Even self-report cannot be relied upon--people are notoriously unable or unwilling to give accurate accounts of the amount of time, say, they spend on various things. And a key to the significance of a type of television program may not be in the amount of time the family set is on, but in the family pattern of speaking around it. Is the set on, but ignored? Does someone insist on and get silence? Is the program essentially a resource for continuing conversation?

All that I have said is compatible with a generous view of scientific method. The subject-matter of ethnography--people and their worlds--imposes conditions such that validity and research design have a complexity and openness at the other end of the scale from experimental design in many fields. Even so, there is a similarity to the problems of fields such as astronomy and geology in their observational aspects. In principle, a sufficient accumulation of valid knowledge about a particular society would make possible rather efficient, precise inquiries of an experimental or quasi-experimental sort. Indeed, there are some cases of this sort. My own experience is with languages. Given the accumulated knowledge of the Native American language I have studied, it is possible to address many particular questions rapidly, systematically, precisely. If a new word is in question, it is a matter of minutes to establish its grammatical place in the system. One does know already

just what questions must be asked, to establish that the word is a noun or verb, say, and just what kind. If a newly discovered recording of a familiar word is in question, one knows exactly what the possibilities of interpretation are, how the facts of the language constrain the sounds that the letters may represent. (All of this presupposes a native speaker who has become accustomed to collaboration in work of this kind). A social anthropologist can look at a newly collected schedule of kinship terms and place the system approximately quite quickly.

IV

These examples illustrate two points made previously--that ethnographic training involves training in the accumulated comparative knowledge of the subject, and that the existence of comprehensive knowledge about a community makes more precise hypothesis-testing possible.

To leave matters here, however, might suggest that ethnography can become almost equivalent to laboratory work. The theoretical foundations on which it rests, however, have yet to prove as certain as that. The social knowledge that ethnography serves is in the paradoxical position of becoming increasingly certain of more and more, and yet at the same time vulnerable to dispute about its very foundations. Our last example, that of kinship terms, is an excellent case in point. No one could deny the striking progress in our ability to recognize and describe the terms and behaviors relevant to kinship in the hundred years or so since Morgan's pioneering work. Descriptive technique, comparative typology, even mathematical modelling are well advanced. Yet it is possible to dispute the correct interpretation of particular findings (cf. Blu 1967) and even the correct definition

of the domain of "kinship" itself (cf. Schneider 1972; Geertz and Geertz 1975). Differences in analytic point of view can take different vantage points within a shared body of data (e.g., Blu), and even prescribe different definitional constructs for master-concepts such as the "cultural" and "social," such as to require different placements of findings.

Notice that such disputes presuppose the success of ethnography. Ethnography has provided what there is to dispute. To be sure, new theory can bring out new aspects of old data, or point up its limitations, or require new kinds of data to develop adequately. Still, such disputes presuppose a great deal of valid first-order data. And they tacitly assume that valid first-order data is largely a matter of competence and talent, requiring perceptiveness and imaginative projection, to be sure, but not miracles of rapport and identification. The native's point of view can be grasped by someone who does not always like it or them (cf. Geertz 1976).

In sum, ethnographers have available in many areas a first-order language of description that permits them to do work that can be judged for its competence, validity, richness, and the like. At the same time they work in a discipline whose second-order language of analysis is contested. A set of native terms may be accepted, but analysts argue as to the priority of one or another part of the data, or as to the scope of the data to be considered, or as to whether the case does or does not fit a certain concept or type. Some of these disputes can be resolved with increasing attention and precision. Others depend on conscious or unconscious commitments to what it means to be a scholar or scientist, and to what the world is or should be, such that resolution is unlikely.

One can look at a schedule of kinship terms, then, and place it approximately quite quickly. But "approximately" is a crucial term. Others may agree, that is a Crow system, and add, "But...". The ethnography of schooling and education no doubt will have the same experience. We can hope to reach the point at which anyone can look at a body of data and say quite quickly, "Ah, that is a Henry Lea type of school," or the equivalent; or say, that community or that family has a Hopi type of educational process. We can probably not hope to reach the point at which no one will object, but you analyzed the school in isolation from X, or started the analysis from Y instead of Z.

There is a second question of language which may differentiate ethnography from the ideal conception of an experimental science. Some of what we believe we know about cultural patterns and worlds is interpretable in terms of structure, whether the ingredients of the structure be lines, graphs, numbers, letters, or abstract terms. Some of what we believe we know resists interpretation in terms of structure. It seems to require, instead, <u>presentation</u>.

The need for <u>presentation</u> seems to cause no comment when the presentation is visual. "A picture is worth a thousand words" and all that. Especially when the object of analysis is material culture, visual presentation is accepted as indeed essential. Even with social life, there is an increasing recognition of the value of visual presentation, through photographs and films. A telling account of necessity here was made by Frederick Barth, the Norwegian anthropologist, when he reported that in order to make good sense of a deceased colleague's field notes, he had to go to the place in question. He had worked ethnographically in the region (which is why he was asked to interpret the notes for publication, but still needed to <u>see</u>

the land, the distances between dwelling and well, the heights and contours of the place--so much of the spatial configuration of life was taken for granted in the notes on behavior.

The difficulty with presentation seems to arise when the presentation is verbal. What is one to make, for example, of the relation between the two parts of Clifford Geertz' "The Balinese cockfight"--one part narrative, one part analytic (1972). Geertz thinks that both parts are important.[3] I do also. Through his narrative skill, he is able to convey a sense (mediated by his personal involvement) of the quality and texture of Balinese fascination with cock fighting. Evidence of the fascination is important. It supports taking the activity as a key to something essential about the Balinese, it helps us understand the analytic statements. A film might help too, but it would need something verbal from Geertz to teach us what we should learn from it. The narrative part of Geertz' article in effect points, as the narrator of a film might do, and, also, in the absence of a film, shows. It does so through texture and proportion.

Many anthropologists agree that something of value can be learned from novels of certain sorts, and even recommend certain novels. Clearly there is a sense in which narrative can be a source of knowledge. For some scientists and philosophers of science, it is a source of knowledge secondary to others, if not in principle reducible to others. Some ethnographers and philosophers of science hold the contrary. Narrative does not seem to them in principle entirely reducible to other forms of knowledge, but fundamental in its own right. Indeed, they may suspect that narrative accounts play a role in what scientists and administrators believe

themselves to know, even though some of these may not acknowledge that role. It may be the case that structural forms of knowledge about social life are usually interpreted, even if covertly, in terms of images of kinds of person and situation, implicit or remembered narratives. If so (and I think it is so), the general problem of social knowledge is two-edged: both to increase the accumulated structural knowledge of social life, moving from narrative to structurally precise accounts, as we have commonly understood the progress of science; and to bring to light the ineradicable role of narrative accounts. Instead of thinking of narrative accounts as an early stage that in principle will be replaced, we may need to think of them as a permanent stage, whose principles are little understood, and whose role may increase. How often, one wonders, are decisions reached on the basis not only of numbers and experiments, but also on the basis of privileged personal accounts, fleshing out the data to make it intellible? Sometimes these accounts may be provided by the investigator, sometimes by the audience ("I knew a case once...."). Sometimes they may not be articulated, yet influential nevertheless.

If narrative accounts have an ineradicable role, this need not be considered a flaw. The problem is not to try to eliminate them, but to discover how to assess them. What criteria can we provide equivalent to the criteria for assessing the significance and validity and reliability of statistical tests, and experimental designs?

The question of narrative brings us to another aspect of ethnography. It is continuous with ordinary life. Much of what we seek to find out in ethnography is knowledge that others already have. Our ability to learn ethnographically is an

extension of what every human being must do, that is, learn the meanings, norms, patterns of a way of life. From a narrow view of science, this fact may be thought unfortunate. True objectivity may be thought to be undermined. But there is no way to avoid the fact that the ethnographer himself or herself is a factor in the inquiry. Without the general human capacity to learn culture, the inquiry would be impossible. The particular characteristics of the ethnographer are themselves an instrument of the inquiry, for both good and bad. For good, it is important to stress, because the age, sex, race, talents of the ethnographer may make some knowledge accessible that would be difficult of access to another. For bad, as we all recognize, because of partiality. Since partiality cannot be avoided, the only solution is to face up to it, to compensate for it as much as possible, to allow for it in interpretation. The conditions of trust and confidence that good ethnography requires (if it is to gain access to valid knowledge of meanings) make it impossible to take as a goal the role of impartial observer. The normal people from whom one has to learn will not put up with that. In principle, the answer lies in the view taken by Russell Ackoff, that scientific objectivity resides, not in the individual scientist, but in the community of scientists. That community has provided methods which to some degree, often a great degree, discipline the investigator and overcome partiality; the rest is a responsibility of critical analysis within the community.

The fact that good ethnography entails trust and confidence; that it requires some narrative accounting; and that it is an extension of a universal form of per-

sonal knowledge, make me think that ethnography is peculiarly appropriate to a democratic society. It could of course be reduced to a technique for the manipulation of masses by an elite. As envisioned here, ethnography has the potentiality for helping to overcome division of society into those who know and those who are known.

Such a vision of a democratic society would see ethnography as a general possession, although differentially cultivated. At one pole would be a certain number of persons trained in ethnography as a profession. At the other pole would be the general population, respected (on this view of ethnography) as having a knowledge of their worlds, intricate and subtle in many ways (consider the intricacy and subtlety of any normal person's knowledge of language), and as having necessarily come to this knowledge by a process ethnographic in character. In between--and one would seek to make this middle group as nearly coextensive with the whole as possible--would be those able to combine some disciplined understanding of ethnographic inquiry with the pursuit of their vocation whatever that might be. From the standpoint of education, obviously one wants to consider the possibility of adding ethnographic inquiry to the competencies of principals, teachers, and others involved with schools. But on the one hand, there is no reason not to seek to extend a knowledge of ethnographic inquiry to everyone. And, on the other hand, there is no reason to think professional ethnographers privileged. In their own lives they are in the same situation as the rest--needing to make sense out of a family situation, a departmental situation, a community situation, as best they can.

If this account sounds a little like a form of conscious-raising, perhaps it is, but it is not the ordinary sort. In this sketch I have not brought out the

sociocultural substantiality of ethnographic inquiry. It is a mode of inquiry that carries with it a substantial content. Whatever one's focus of inquiry, as a matter of course one takes into account the local form of general properties of social life--patterns of role and status, rights and duties, differential command of resources, transmitted values, environmental constraints. It locates the local situation in space, time, and kind, and discovers its particular form and center of gravity, as it were, for the maintenance of social order and the satisfaction of expressive impulse.

It is for this reason that much observational analysis of classrooms does not seem to me to merit the term "ethnography." On the one hand, there is a kind of work which consists essentially of recurrent observations according to a pre-established system of coding. Such work violates the principle of being open to discovering meanings and patterns of behavior not foreseen. There is no provision for meanings, and patterns are excluded, since integral stretches of behavior are not observed. A superior kind of work analyzes intensively integral sequences of behavior. It contributes greatly to analytic control and to penetration to underlying meanings and connections. The limitation of the work is the lack of a comparative perspective. Thus, Ray McDermott (1977) interprets the difficulties, and projected failure, of one group of first-grade readers, in terms of the mode of interaction among them and the teacher (in contrast to the mode of interaction of another group with the teacher). Since his data is limited to the classroom, he is not able to consider differences in mode of interaction that the children may have brought with them to the classroom. A comprehensive ethnography would consider all the types of scenes in which the children

(and teacher) participate, in order to assess validly the meaning of the behavior in one scene. From a larger point of view, the lack of a comparative ethnological perspective weakens the contribution the research might make. Suppose it is the case that what happens in the first-grade classroom is going to determine the success or failure of the children as readers, and ultimately, as adults. (This is McDermott's view.) We have to ask whether or not such a circumstance is inevitable. Some Third World societies have had great success with literacy programs, through mobilization of the society to accomplish it. Is it impossible for the United States to give that kind of priority to universal literacy among its citizens, and mobilize to accomplish it? Is the fundamental question not this: how does it come about that one society, and not another, lets literacy depend on patterns of interaction in first-grade classrooms?

Such an interdependence between general and particular inquiry is essential to ethnography as a mode of inquiry--at least it is essential in my reading of the history of anthropology, and to what I would see as the contribution ethnography ought to make to education.

Dell Hymes

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2 Petty outlined "A method of enquiring into the state of any country" and in 1686 a series of questions concerning "The nature of the Indians of Pennsylvania" (Hodgen 1964: 190; Slotkin 1965: 481, n. 363, where the title is given as "Quaeries concerning the nature of the natives of Pennsylvania"). Petty's questions were part demography--he has been called "the greatest exponent of social statistics in the seventeenth century" (Slotkin 139), part ethnology. He was man who both recommended that the Royal Society admit only words that mark number, weight or measure, and who was concerned with clearer definitions of ethnological entities. Altogether a worthy forefather for any effort to integrate quantitative and qualitative methods today. His program of ethnological investigation was similar to what Roger Williams (1643) has actually done some forty years earlier among Indians, but still quite unusual for its time. By the eighteenth century serious frames of reference for the collection and interpretation cultural facts ("manners and customs") were to become part of an established tradition, as in the Scottish work of Adam Ferguson, Lord Kames, James Millar and William Robertson.

3 Geertz (1975) expresses superbly the dialectic between the two orders of analysis, the descriptive and the generalizing, that requires concern with adequacy of "presentation." Thus, he writes (p. 223):

> "Confinement to experience-near concepts leaves an ethnographer awash in immediacies, as well as entangled in vernacular. Confinement to experience-distant ones leaves him stranded in abstractions and smothered in jargon. The real question...is... how, in each case, ought one to deploy /the two sorts of concepts/ ...so as to produce an interpretation of the way a people lives which is neither imprisoned within their mental horizons...nor systematically deaf to the distinctive tonalities of their existence."

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